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A PRELIMINARY report of the investigation of the upper air in Java has recently been made by Dr. W. van Bemmelen and Dr. C. Braak. Aerological investigation at the Batavia observatory was begun under the auspices of the Dutch government in 1909. Because of the proximity of the sea, pilot balloons only were used at first, and with these a more thorough knowledge of the upper currents was obtained. Later recording instruments were elevated by means of captive balloons and kites, the latter being used above the sea as well as above the land. It was found that during the period September–May the general air-current had easterly components up to the greatest heights attained (10–15 kilometers), though occasionally the west monsoon appeared at the ground, its average height having been found to be 5.4 kilometers. No antitrade wind aloft was found. However, on one occasion when a balloon attained a height of 18 kilometers it encountered a westerly wind, similar to the strong westerly winds which were observed at heights of 10–20 kilometers on Professor Berson's East-African expedition. This phenomenon still awaits an explanation.

THE newly created professorship in meteorology at the National University at Utrecht has been awarded to Dr. E. van Everdingen, who assumed the chair October 17. Considering the recent history of meteorology, the inaugural address, "The Third Dimension in Meteorology," was particularly appropriate. In Dr. Everdingen's estimation, the setting apart of a chair of meteorology indicated a recognition that meteorology was now worthy of a place among the established sciences.

As a result of many requests from teachers, students and others interested in the subject, the Weather Bureau has published a second compilation of standard books dealing with meteorology and its several branches. The list includes about 150 titles, the selections having been made by Mr. C. Fitzhugh Talman, librarian of the bureau. As stated in the introduction, "the present compilation is the fruit of several years' experience in dealing with the literature of the subject, and will probably meet the requirements of the majority of American readers and students."

THE action of the management of the recent International Aviation Meet at Belmont Park in taking out insurance against loss due to inclement weather is one of the first instances of its kind in America. The practise is a common one in Europe, however, especially so in England, where managers of most of the outdoor gatherings have long insured through Lloyd's against loss from wet weather. The premiums paid for the risks were relatively large at first, but of late there has been a tendency toward placing the practise upon a scientific basis, statistics having been gathered with that end in view, and in consequence the rates have been readjusted.

ANDREW H. PALMER
BLUE HILL OBSERVATORY,
January 14, 1911

SPECIAL ARTICLES

INTERPRETATIONS OF RESULTS NOTED IN EXPERIMENTS UPON CEREAL CROPPING METHODS AFTER SOIL STERILIZATION[1]

IT is not my intention at this time to give the details of extended experiments upon soil sterilization and its effects; nor to enter any special criticisms upon the work of other investigators. I wish only to call attention to some facts, observations and conditions of the work centered about cereal cropping and experiments upon soils which may indicate that a new light may be thrown upon the conclusions to be drawn; with that light emanating from a different source than has usually been indicated by most experimenters.

Observations and Reflections.—The following features of cropping and experiments will be familiarly known to most of you:

1. New Lands, when first sown to wheat or other cereals, produce quite lavishly in seed of high quality and at slight effort on the part of the farmer. These new land yields, in this country, are quite commonly taken as the standard of what ought to be expected.

[1] Read before the Society of Agronomy, Washington, D. C., November meeting, 1910.

2. It is a common experience that as soon as a particular cereal crop has become general, and that usually follows in a very few years, a marked deterioration, both in yield and quality, sets in. The crop, except in special years, and under rare exceptions of special farming, seldom again reaches the same high grade of yield and quality. Indeed, the yield generally falls to the average for the country, above which it can be raised again only through exceptional methods; and, to the chagrin of many of our most able agricultural educators, no philosophy of cropping or land improvement seems to give the farmer the desired results with any regularity, year by year, for any long period of time. The crop or variety once a favorite in a locality usually has a short life and finally gives place to a real change in agriculture, seldom, if ever, to regain its place.

3. Not many theories have been advanced to account for these results. The chemist and his followers have usually directed thought in the matter, and agriculturists, generally, have taken the chemist's dictum that marked changes have occurred in the balance of plant food relations of the soil, thus accounting for the rapid first deterioration of the crop through chemical losses noticed in the soil. Thus if a lack of proteid is found in the grain of wheat and a loss of nitrogen is observed in the soil, it has been reasoned, without foundation, I think, that the noticed chemical loss in the soil is necessarily the cause of the deficiency in the kernel. When our chemical friends have, by their own analysis, discovered that there is, however, sufficient strength of soil solution regarding all known necessary chemical elements to support a crop on a particular field, the failure to reach crop quality has been quite uniformly attributed, by them and the rest of us, to slovenly methods of farming, poor physical texture of the soil, degenerated seed, etc.

Any other special theories which have been advanced in particular to account for the facts have all been strongly influenced by the recognized fact that soil can be impoverished, reduced in its chemical strength. The Whitney toxine theory would appear to be only a reflection of this troubled state of the chemical and physical mind, associated with a desire to show that a complex plant growing in the soil and air acts upon the soil after the manner of a bacterial culture in a test tube. That I may not be misunderstood, I may say that I believe that certain soils may be exhausted chemically by cropping methods; that I think it is wholly possible that the excrementia of plants under rather constant cropping may have an analogous effect upon the crop to that noted in bacterial cultures upon the substratum, but that after several years of careful trials upon wheat and flax, both under culture house conditions, and under carefully planned plot trials, I have been unable to find any point which would tend to substantiate the toxine theory. Nevertheless, the contention of Mr. Whitney, that the soils of cereal regions are not particularly exhausted is, in my belief, much nearer to the truth than the contention of the chemists and others that the deteriorated yields and qualities of wheat and other cereals are due to chemical exhaustion, and especially to nitrogenous exhaustion; for neither the chemists' exhaustion theory nor the toxine theory can account, to my satisfaction, for the failure of virgin soils to produce the yields characteristic of that region when such cereal cropping was first introduced. It is a fact that such lands are quite as liable to give the crop characteristic of the old, so-called, worn-out lands, as do the older lands. It is not the uniform failure of the particular crop which causes it to be dropped by a farming community, for it is evident that all of the lands of a community can not be so depleted. It is the general uncertainty of giving results, year by year, which results in abandoning or ceasing to expect a proper yield. It is evident from the foregoing considerations that there are constant interfering agencies at work in cereal cropping regions which have not as yet been properly taken into consideration, for, even under the best weather conditions possible, essentially the same weather conditions which in a new

land region give fine yields, often the crop fails to give both quantity and quality even under our best planned systems of rotation and of soil fertilization.

4. Experiments in soil sterilization applied to such old and supposedly deteriorated soils have uniformly given quite marked improvement in results. The results have been so uniformly good, whether done by steam or by chemical methods, that one or other practise has become general with the glass house gardeners and seedling plant producers. They seem, long ago, to have realized what sterilization of soil has done for them, but experimenters upon field crops still look for explanation for such improvements.

5. Two very interesting explanations of such effects of sterilization, both based upon carefully planned and executed experiments, have lately been attempted; and, as my experiments cover essentially the same fields of effort, and, when published, will show almost exactly the same results but quite different conclusions, I may be pardoned, at this time, for outlining these three sets of experiments and the results, with some slight comment upon the conclusions:

Mr. A. D. Hall, of Rothamsted, England, in SCIENCE, September 16, 1910, reports upon experiments conducted at the Rothamsted farm.

Speaking of wheat, he says:

> Approximately the crop becomes double if the soil has been first heated to a temperature of 70° to 100°, for two hours, while treatment for forty-eight hours with the vapor of toluene, chloroform, etc., followed by a complete volatilization of the antiseptic, brings about an increase of thirty per cent., or so. Moreover, when the material so grown is analyzed, the plants are found to have taken very much larger quantities of nitrogen and other plant foods from the treated soil; hence, the increase of growth must be due to larger nutriment and not to mere stimulus.
>
> The explanation, however, remained in doubt until it has been recently called up by Drs. Russell and Hutchinson, working in the Rothamsted Laboratory. In the first place, they found the soil, which had been put through the treatment, was chemically characterized by an exceptional accumulation of ammonia to an extent that would account for the increased fertility. At the same time it was found that the treatment did not effect complete sterilization. . . .
>
> The question now remaining was, what had given this tremendous stimulus to the multiplication of the ammonia-making bacteria? By various steps, which need not here be enumerated, the two investigators reached the conclusion that the cause was not to be sought in any stimulus supplied by the heating process, but that the normal soil contained some negative factor which limited the multiplication of the bacteria therein.
>
> Examinations along these lines then showed that all soils contain unsuspected groups of large organisms, of the protozoa class, which feed upon living bacteria. These are killed off by heating, or treatment by antiseptics, and on their removal, the bacteria, which partially escape the treatment, are now relieved from attack. . . .
>
> Curiously enough, one of the most striking of the larger organisms is amœba.

The authors, Messrs. Russell and Hutchinson, thus attempt to account for the greater wheat crop production of soil sterilization both through chemicals and through steaming, by a reverse application of the Metchnikoff theory. It would be unwise of me, not knowing all of their data or having access to the soil or the seed which they used, to enter a criticism, but from my own observations and work, I can not agree to any of the conclusions which are drawn in these paragraphs. So far as Mr. Hall has made plain in SCIENCE, they can only be matters of inference, and many conditions could enter, which would vitiate the necessity of assuming the detrimental rôle for the amœba. For example, the authors do not explain why their sterilization did not sterilize, and what happened when they did really sterilize the soil. In order to clarify the theory as proposed by Dr. Hall, it would seem necessary to try real sterilization, both upon the amœba and the supposedly beneficial bacteria.

It is quite possible that the production of ammonia in soils by bacteria is a beneficial process, but I can not say wherein this theory would rest, if one should assume the presence of plenty of ammonia and plenty of ordinary nitrates in the soil. In such case, if the soil still failed to produce wheat, and proper ster-

ilization succeeded in making it produce wheat, their theory would seem to be without ground. My experiments in sterilization result in either good or bad wheat according to what I do to the seed planted therein, though there can not be any question but what in some soils increased amounts of ammonia through sterilization do have something to do with the results.

Experiment by Professor T. L. Lyon, of Cornell University, Bulletin 275, "Upon the Effect of Steam Sterilization on the Water-soluble Matter in Soils," attempts an explanation of the peculiarities of growth of the wheat plants upon soils after steam sterilization through differences in the soluble content of the soil resulting in differences in density of the soil solutions, etc. He also, however, seems to have great difficulty in accounting for some of the peculiar actions of the growing wheat plant upon such treated soils and solutions, especially in explaining what appears to be a really injurious effect upon the first growth from the seedlings, though finally followed by actual increase in crop.

In our experiments, we are able to explain most of these peculiarities of growth, noticed both in our cultures and those of Professor Lyon's admirably conducted trials, upon a biological relation of the wheat plant to certain actual disease-producing organisms and their growth relations to the crop plant, and to the various interreacting soil relations, which react both upon the crop plant and upon the disease producers.

In our experiments we find that both soil and seed may be, and usually are, infected by several very destructive wheat-destroying, parasitic fungi. Indeed, these are found to be apparently cosmopolitan in distribution with the wheat plant. They are especially transmitted in the seed internally, and, it seems quite certain, are sufficient in their influences to account for most of the causes of rapid first-crop deterioration, and for the difficulty which farmers have in introducing any sort of culture, which will again raise the standard of crop. Their exclusion, in so far as it is perfectly or imperfectly done, is sufficient to account for the anomalies indicated in soil sterilization experiments. However, in our experiments our results and conclusions have always been vitiated whenever these fungi were not eliminated.

I do not question that soil sterilization does change the bacterial content or that it does influence the soluble content of soils, but I am inclined to think that disease-infected seed and disease-infected soil will eventually be found to have much more to do with the irregularly corresponding conclusions, which have been drawn by various experimenters upon crop rotations, upon soil-fertilization experiments and upon soil-disinfection experiments than they have ever suspected. Indeed, I have but slight doubt that the whole theory of auto-intoxication (toxine theory) as applied to cropping plants, is virtually vitiated in its conclusions, because of a lack in eliminating the influences of pathogenic organisms in the experiments.

H. L. Bolley

Agricultural College,
North Dakota,
November 1, 1910

Tertiary Deposits of Northeastern Mexico

During the past two years, the geologic work under my direction in southwest Texas and northeast Mexico has resulted in the accumulation of a mass of information which materially adds to our knowledge of the Gulf Tertiaries. The fieldwork was carried on by Messrs. W. F. Cummins and W. Kennedy, assisted by Mr. J. M. Sands.

The first year's work by Professor Cummins was a general examination of the northeastern Mexico for artesian water. Following this, I had a careful section made of the Cretaceous and Tertiary deposits along the Rio Grande, and then traced the contact between the two systems southward into Mexico as far as this could be done within the scope permitted by our economic work. The widespread occurrence of the different phases of the Reynosa formation prevented direct connections of the exposures of the underlying deposits in some places, but we were able to